Atmos. Chem. Phys. Discuss., 8, S9081–S9084, 2008
www.atmos-chem-phys-discuss.net/8/S9081/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the validity of representing hurricanes as Carnot heat engine" *by* A. M. Makarieva et al.

Anonymous Referee #2

Received and published: 13 November 2008

When describing large scale circulation (e.g., Hadley circulation) conventional meteorology places the spatial temperature gradient as the physical cause behind the pressure gradient force. That is, surface temperature forces air, making it lighter (warmer) or heavier (colder), inducing vertical mass movements to accommodate for ensuing buoyancies. Density driven local air motion results in horizontal pressure gradient at larger scales, in this way driving regional circulation (see discussion in HESSD [1], especially Dovgaluk S1135-S1137). In this framework, solar radiation propels air motion in the atmosphere mostly by differentially heating the Earth surface, and that is the basis upon which all further understanding of atmospheric air motion is built. However, a number of circulation circumstances defy this physical logic to aptly explain atmospheric circulation. In the HESSD discussion [1], as well as in the present dis-

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cussion, the contrast between ocean and neighboring land surface temperatures has been indicated as a puzzle for the conventional circulation framework, that produces circulation paradoxes like, e.g., the persistent "temperature-countergradient" air flow from the warmer Atlantic to the cooler Amazon basin. More to the point of the DP, the approach to hurricane description that the authors criticize presumes that the highest wind speeds observed on the planet develop in the absence of surface temperature gradient at all – in Emanuel's framework air accelerates along the isothermal sea surface. Seen at large, the current view on the driving forces behind atmospheric motions appears as physically controversial. Is horizontal temperature gradient at all important for circulation generation to the degree it is currently implemented in GCMs?

In contrast, the present authors offer with their expanded section 4 (see ACPD S8904) a unified and coherent physical picture. Their evaporative force equally explains hurricanes and the slower, large-scale circulation. It is proposed and physically substantiated that all circulation phenomena arise due to condensation of water vapor. The implication is that the spatial pattern of condensation intensity may or may not be directly influenced by temperature distributions, as it depends on a variety of other factors, like the evaporation rate or surface heterogeneity. The power of such new physical framework lies in its capacity to logically predict and explain a wide range of circulation patterns. Current understanding all too often evades logical explanation of common phenomena with the excuse that the Earth System is too complex. With this new theoretical framework, in my view, the authors have laid down the physical fundaments for a complete reformulation of understanding of atmosphere circulation. This finding cannot and should not be underestimated.

In hindsight, this new development makes the original main objective of the DP (a critique of the physics in hurricane models) dwindle in importance. During this discussion it became evident that peers siding with the prevalent hurricane theory could neither read objectively the authors clear physical arguments, nor dedicate the proper attention in their reviews and comments to the novel theory offered in the DP and further in the

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extensive comments and responses from the authors. Most peer questioning, explicitly and painstakingly answered by the authors, ended up without a conclusive analysis by the critics, that could be potentially useful for editorial decisions respecting the DP. My reading of these attitudes suggests that this representative part of the community, aligned with the status quo in meteorology, reacted defensively against what might have been perceived as a naked attack to established science, received at first also as an empty or vague criticism. But the fast paced marathon of responses provided by the authors left no question unanswered, with their answers clarifying doubts, dispelling critic mistakes and falling neatly and coherently within the logical framework of their new theory (which can only be properly evaluated through the competent analysis of the whole framework, as in this new section 4). The authors have demonstrated extraordinary patience and rare receptivity to all questionings. But their straight approach has shown no atavism to "handbooks" of any sort. Their arguments stood solely on fundamental principles of physics; therefore any productive critique must follow the same path. NEW discoveries in science cannot logically submit to what established mainstream handbooks teach. Engineering finds good use in handbooks, while fundamental science worries about producing the knowledge upon which new handbooks will be written. The fact that, it appears, some peer critics remain disagreeing that the proposed new physical understanding brings a fundamental new contribution to science, loses in credibility by the evident [in some cases] and self defeating admission [in other case] that neither the literature nor the discussion have been properly followed. In my view, and it appears also in the view of supporting peers, the self standing brilliant contribution of these hard working authors should not and must not be penalized by incomplete review or by lousy critique. In any circumstance, what this DP and the present discussion already brought to the fore, potentially powerful physical explanations of the atmosphere emerging from a fundamentally different and refreshing approach to the atmospheric science, is a permanent gain that will inspire new approaches and spur fertile new thinking years to come. This original contribution alone should justify publication in ACP.

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To foster the constructive side of this DP, and to take advantage from the detailed explanations and valuable additional science offered by the authors in the present discussion, I suggest the following reorganization of the paper structure for publication:

Title: consider changing to something suitable to describe the new theory

1. Shortened critique

2. Longer Section 4 including the authors comment on Condensation as air circulation driver (S8904) (plus the two appendices there)

3. Consider including some part of the first comment (S7325) in this discussion [2], especially the better explained consideration of the Carnot cycle, as an appendix.

In conclusion, I congratulate the authors and all discussion participants for the vibrant exchange of ideas. From the new theory one starts to wonder about exciting implications, like explaining dust devils on Mars (where water vapor, although present in tiny quantities, also undergoes phase transitions).

[1] A.M. Makarieva, V. G. Gorshkov Interactive Discussion Biotic pump of atmospheric moisture as driver of the hydrological cycle on land, Hydrol. Earth Syst. Sci. Discuss., 3, 2621-2673, 2006

[2] A. M. Makarieva, V. G. Gorshkov, and B.-L. Li On the validity of representing hurricanes as Carnot heat engine, Atmos. Chem. Phys. Discuss., 8, S7325–S7335, 2008

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17423, 2008.

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